JUSTUS-LIEBIG-UNIVERSITÄT GIESSEN

Finanzwissenschaftliche Arbeitspapiere

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Institutional choice in social dilemmas – an experimental approach

Arbeitspapier Nr. 63/2002

ISSN 0179-2806

Fachbereich Wirtschaftswissenschaften

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Abstract

This paper presents an experimental study on the ability of human groups to escape the tragedy of the commons through institutional change. It shows that the groups identify institutional change as a means of resolving social dilemmas and are ready to apply it even if the change requires an unanimous vote. At the same time, the groups who were given the right to change the rules performed poorer on average than the control-groups. This result stands in contradiction to elementary economic reasoning as well as the results of previous experimental studies.

Key-words: social dilemmas, common pool resource, laboratory experiment, group behavior, institutional choice

JEL: C92, D71, D62, Q20

* The author would like to thank the "Förderverein des Fachbereichs Wirtschaftswissenschaften der Justus-Liebig-Universität Gießen" for financial support.

1. Introduction

Since Hardin (1968)'s tragedy of the commons, social dilemmas have been on the research agenda of social science. Social dilemma situations typically appear when a number of individuals have free access to a scarce common pool resource (CPR) or when they have to provide a public good on basis of voluntary contributions (e.g., *Olson*, 1965; *Ostrom*, 1990). In these cases, it is individually rational to make excessive use of the CPR respectively not to contribute any private resources to the provision of the public good, in short to defect. Such defectious behavior results in individual as well as overall welfare losses compared to a situation where all individuals cooperate. Previous research has shown that human societies face social dilemma situations in many fields of their economic life. At the same time, they have proved to be very creative in finding ways to resolve these dilemma situatons (e.g., *Ostrom*, 1990; *Heltberg*, 2002). Following *Messick and Brewer* (1983), they apply two general categories of solutions. First, the individuals can use moral suasion and threats to convince each other to cooperate while leaving the institutional framework of their interaction unchanged. Alternatively, they can try to change precisely this framework in order to alter the incentive structure in a way that makes cooperative behavior individually rational.

The applicability and effectiveness of these two options is the subject of numerous different studies. Recently, a number of authors have applied laboratory experiment to contribute to this body of research. Depending on the options the players have in the experiment, two different types of experiments can be identified. The vast majority of experiments only allows the individual player to choose between different actions within a given set of rules. The primary objective of these single-choice experiments is to find out to what extent individuals cooperate in social dilemma situations and what factors determine the degree of cooperation (e.g., *Isaac, Walker and Thomas*, 1984, 1994; *Ostrom, Walker and Gardner*, 1992). The second type of experiments will be called double-choice experiments. Like in single-choice experiments, the individual players can choose between different actions. Additionally, the group of players as a whole can change the institutional restrictions on the individual choice of action. Double-choice experiments aim at answering the question under what circumstances institutional change is introduced and whether this step improves efficiency. So far, only very few double-choice experiments have been performed (e.g., *Samuelson and Messick*, 1986; *Carpenter*, 2000).

This paper presents a newly designed double-choice experiment and reports on its results. Section 2 gives a short overview on previous experiments on human behaviour in social dilemma situations with a special focus on double-choice experiments. Section 3 outlines the set-up of the experimental study presented in this paper. It differs from previous double-choice experiments in three central aspects. First, the teams are not given the chance to learn about the mechanisms of the game before being allowed to change the institutional settings. Second, the individuals are allowed to communicate. Third, the current experiment drops the assumption according to which institutional restrictions can be enforced at zero costs. Section 4 lays out the predictive theory. The results of the experiments are presented in section 5 and discussed in section 6.

2. Previous experiments on social dilemma situations

The number of single-choice experiments on social dilemma situations is large. They differ in some major characteristics of their set-up, such as group size, form and level of payoffs or the extent to which the players are allowed to communicate. Most of them are so-called public good games, in which a group of individuals are given the task to provide a public good on the basis of voluntary contributions (e.g., *Marvell and Ames*, 1979; *Isaac, Walker and Thomas*, 1984, 1994; *Palfrey and Prisbrey*, 1997). In addition, some CPR-experiments are reported. These assemble a number of players around a CPR, which they have to cultivate (e.g., *Gardner, Moore and Ostrom*, 1987; *Ostrom, Walker and Gardner*, 1992; *Walker et al.*, 2000). Regardless of their actual set-up, all single-choice experiments report an average degree of cooperation which is below the group-efficient degree but substantially above the one predicted by economic theory (e.g., *Isaac, Walker and Thomas*, 1984; *Ostrom, Walker and Gardner*, 1992; *Palfrey and Prisbrey*, 1997; *Walker et al.*, 2000). The level of cooperation rises further if players are given the possibility to communicate (e.g., *Ostrom, Walker and Gardner*, 1992; *Weimann*, 1994) or to impose sanctions on defecting players (e.g., *Chen and Plott*, 1986; *Fehr and Gächter*, 2000).

Compared to the numerous single-choice experiments, the number of double-choice experiments is very small. One series of experiments on institutional choice was performed at the University of Santa Barbara and Groningen (*Samuelson et al.*, 1984; *Samuelson and Messick*, 1986). Therein the subjects are told that they are part of a group of six individuals who cultivate a CPR over a number of rounds. In reality, they do not play in groups but every individual participates in an isolated experiments in which the other members of the groups are simulated. In every round, the human player is informed about the initial pool size and is asked to state the amount of resource he wants to extract in this round. The player gets false feedback on the pool size. Three different structures are used. The overuse condition confronts the player with a pool which continuously and substantially shrinks over time. In the optimal use condition, the pool size does not decrease systematically but is only subject to small variations. Finally, the underuse condition presents the player a full pool at the end of every round. In all these conditions, the pool size does not depend on the extraction strategy of the human players, but is precisely determined beforehand. After ten rounds, the first session of the experiment ends. The player is confronted with the (simulated) payoffs of all six group members. He is told that some group members have expressed the belief that they could have done better if they had restricted the access to the common pool. Some players are given the opportunity to restrict the access by fully privatising the CPR before continuing to play (privatisation-scenario). The institutional setting chosen depends on the choice of the human player. Other players are told that their co-players suggested to elect a leader who decides on the total harvest and its allocation to the group members (leadership-scenario). If the human player votes in favor of this option, he is told that his co-players have reached the same decision and furthermore elected him to be the leader. The game continues for another ten rounds.

The results of the experiment can be summarized as follows (*Samuelson et al.*, 1984, S. 101). In part 1 of the experiment, the overuse condition led human players to extract significantly less from the common pool than under the other two conditions. The voting behavior in the beginning of part 2 also differed significantly between the conditions. While 66.7 % of the human players under the overuse condition voted for the institutional change, change were introduced only in 25.4 % of the experiments under the other two conditions. Regardless of the condition, the frequency of institutional change was higher under the leadership-scenario (60.4 %) than under the privatisation-scenario (45.2 %). Unfortunately, neither *Samuelson et al.* (1984) nor *Samuelson and Messick* (1986) report on the performance in the second session of the game. Therefore it is impossible to say whether the changes in rules led to an increase in overall efficiency.

A second series of experiments was performed by *Sato* (1987). Therein a group of four players cultivate an artificial forest over a number of periods. Before as well as during the experiment, the players do not know who their co-players are and are not given the opportunity to communicate with them. The experiment consists of twelve sessions. In the beginning of every session, the players plant a certain number of seedlings. As time proceeds, these small trees grow. The longer a tree grows, the larger it is and thus the greater the payoff of the player who harvests it. Harvesting is allowed by every player at any time. At the end of the session, the payoffs from harvesting are calculated and the costs for the seedlings are divided among the players. There are two different cost-allocation rules. A number of groups start off with the equality rule according to which the costs are shared equally among all players. Other groups play under the punishment-assigned rule which allocates the full costs to that one player with the highest payoff from harvesting trees. Each of the twelve sessions represents a new game starting with the same number of seedlings. Some groups perform the full twelve sessions under one rule. Other groups are informed about both cost-allocation rules and the chance to change the initial rule in one of the later sessions of the experiment. In session 7 to 12, these groups are asked to vote on the rule under which they want to play the current session. The previously active rule is changed if the majority of players vote in favor of change.

Sato's experiments delivered the following major results. In the first six sessions, the average payoff of groups under the punishment-assigned rule was distinctly higher than that under the equality rule. The difference in payoffs grew even larger because the groups under the first-named rule managed to increase their payoff over the sessions. In the later sessions their average payoff reached more than 90 % of the maximum possible yield but remained below 50 % for the groups who played under the equality rule. Those groups who initially played under the equality rule and were given the chance to change the rule after round 6 installed the punishment-assigned rule in 46 of 54 sessions (85.2 %). Thereby they increased the average payoff substantially. After having learned about the functioning principles of this rule, they reached group payoffs of above 90 % of the maximum possible yield. Groups who started off under the punishment-assigned rule and had the possibility of changing the cost-allocation rule only made use of this possibility in 16 out of 54 sessions (29.6 %). As a result, their average payoff dropped substantially, reaching only 75 % of the maximum yield in the last round (*Sato*, 1987, S. 24 - 29).

The third series of experiments to be presented here was performed by *Carpenter* (2000). Therein two players use a CPR together. Again, the players do not know their co-players and are not given the opportunity to communicate. Other than the experiments outlined above, *Carpenter* does not use a dynamic experimental setup. Instead, one session only consists of one round. The same pair of players plays a number of sessions. In every session, each player chooses whether he wants to extract a large or a small portion of the CPR. The initial payoff matrix resembles a prisoners dilemma. In the last round, the players are given the chance to transform the payoff matrix by choosing a transformation parameter c. They can choose between six different matrices. All of them yield identical payoffs in those cases when both players cooperate but produce different payoffs in the case of defection. The lower the value of c, the stronger are the incentives to defect. For the highest value of c = 1, no player can increase his return by defecting even if the other player cooperates. In the first rounds of *Carpenter*'s experiments, the level of defection was very high and thus the individual and group payoffs were low. When given the chance, 96 % of groups set c = 1 in the last round and thereby destroyed all incentives to defect. This led to a substantially higher level of cooperation and consequently higher payoffs in the final round (*Carpenter*, 2000, S. 676 - 680).

Summing up, the three double-choice experiments presented above place a group of players in a CPR environment and let them play a number of rounds under a fixed set of rules. Thereby the players are given the chance to learn about the basic mechanisms of the game and try to resolve the social dilemma without restricting the access to the CPR. As they are not allowed to communicate, the success is limited. In the second part of the experiment, some groups are given the opportunity to change the rules of the game and play a number of further rounds. The results of these experiments can be summed up as follows: The degree of cooperation and thus the group payoff depends on the rules under which the groups made use of the CPR. Given the opportunity, a substantial number of groups made use of the right to change these rules. The willingness to change the rules strongly depends on the previous economic record. In those cases where the payoff in the first rounds was poor, the rules were changed in the vast majority of cases. As a result, the payoff increased. Those teams who reached high payoffs under the initial rules changed the rules less frequently. In a few cases, groups changed from a "good" to a "bad" rule, like those groups who abolished the punishment-assigned rule and introduced the equality rule in *Sato*'s study. This led to losses in payoff. On average, however, the payoffs reached after the players were given the opportunity of institutional change exceeded those reached in preceding rounds of the experiment. In sum, the presented double-choice experiments support the following conclusion: Human individuals are able to escape social dilemma situations by changing the institutional framework of their interaction if they are given the opportunity. The possibility of institutional changes allows them to increase the efficiency of resource allocation to an extent that cannot be reached without this possibility.

This paper provides a new double-choice experiment. The set-up differs from the preceding experiments in three fundamental characteristics. First, the groups which are allowed to change the rules are not given the opportunity to learn about the basic mechanisms of the game in a first part of the experiment under a fixed set of rules. Instead, institutional change is permitted from the first round on. Second, communication is allowed throughout the entire game. This reduces the probability that some players do not comprehend the experimental set-up and the given incentives. At the same time, the groups are given the chance to resolve the social dilemma within the initially given set of rules rather than by changing the rules. Third, the current experiment drops the implicit assumption of the above experiments according to which the access to the CPR can be restricted at zero costs.[1] In the following experiment, the enforcement of all rules that restrict access to the CPR causes running costs.

[1] *Samuelson et al*. (1986) set the costs of preventing other players except the leader to use the resource in the leadership to zero. The players in Sato's experiments do not have to spend any resources to observe the payoffs of their co-players or assign the costs for the seedlings. In Carpenter's experiments, a change in the transformation parameter can destroy the incentives to defect without reducing the cooperative payoff.

3. Experimental set-up

3.1 Rules

The experiment is set up to resemble the following real-life background.[2] Five families live of the fish they catch in a nearby lake and sell at the local market. For all families, fishing is the only source of income. Every family consists of three generations. Only the second generation engages in fishing and makes all the necessary decisions. After a certain time, this formerly active generation retires and hands over the right to fish and the equipment to the next generation. Provided that the lake is not depleted, the young generation will grant the retired generation a pension. Since the fish does not exist in abundance and reproduces only at a limited rate, a permanent flow of fish can only be guaranteed if each family catches less fish per period than it theoretically could. The families can organize the necessary fishing restriction by setting up a quota of fish every family is allowed to catch. In order to enforce this quota, they can hire an independent patrolling service. The higher the patrolling intensity, the higher the patrolling costs each family has to incur.

The experiment is played by five players, each representing the active generation of one family. The aim of every player is to maximize his total income during the experiment. It consists of the money he can earn when selling his fish on the market (1 $ per fish), minus the patrolling costs plus the pension. During the entire game, there are no restrictions on the communication between players. The fishing will take seven rounds in total. Every player can catch as much as 2 000 fish per round. Initially, the lake contains 10 000 fish. At the beginning of every round, the teams have the possibility to install a quota which settles the number of fish they are allowed to fish in the current round. If three players agree on this quota, it is passed and violations will be punished – provided that the defecting players get caught. Before the fishing starts, the players can change the patrolling rule which sets the probability p of catching a player who tries to defect. Six different levels of patrolling can be chosen (see Table 1). The rule is changed if three/five players vote in favor of the new rule. Else the old rule remains active. Initially, the rule is set to rule 2.

Next, every player decides on the number of fish he intends to catch. His fishing plans can be in accordance with the quota or deviate from it. Once all players

[2] Some authors do not present a real-life background story to avoid framing effects. The major advantage of embedding the incentive structure in a real-life background is that it makes it much easier for the players to discuss their situation and possible solutions. In order to allow the groups to fully exploit the advantages of communication, the current experiment presents a real-life background.

have made their decision they declare their fishing plans to the organizer of the game. The competing players do not get to know each others' fishing plans. If a player defects, his fishing success depends on whether he gets caught or not. For each attempt to defect, a random number processor is used to determine whether it is successful or not. If a player gets caught, he will receive no fish and his attempt to defect will be publicly announced. Otherwise his fishing plans are not reduced and his defection is not be made public.

Table 1: Patrolling rules, costs and probability of catching a defecting player

Patrolling rule	Patrolling costs (C) per player and round	Probability P
1	0	0
2	50	0.25
3	150	0.5
4	250	0.6667
5	450	0.85
6	700	0.95

The total demand for fish consists of the sum of fishing plans of the cooperative players plus the fishing plans of those defecting players who were successful in their attempts to defect. If the total demand for fish is smaller or equal to the contents of the lake, every player (except for those who got caught defecting) will get the demanded amount of fish. If the total demand exceeds the amount of fish in the lake, all fish will be distributed among these players proportionally to their demand. The final amount of fish left in the lake is announced. If the lake contains less than 500 fish, there will be no further fishing and thus the experiment ends here. Otherwise, the remaining number of fish is doubled to give the starting point of the next round. However, the number of fish cannot exceed 10 000. The fishing ends at the latest after seven rounds.

In the eighths round, the players retire and are granted a pension. Every player will receive a single payment amounting to one quarter of the remaining number of fish in the beginning of round 8; at most $ 2 000. In round 8, the players do not have to incur any patrolling costs. The experiment ends here. The organizer informs every player separately about his total income during the experiment. For every $ 1 000, a player is paid 1 EURO in cash.

3.2 Participants

In total, 20 experiments involving 100 students were performed. In six experiments, the players were allowed to change the patrolling rule by simple majority vote (MAJ), in another six experiments an unanimous vote was necessary (UNA).

The remaining eight experiments served as control-group. Therein the players were neither allowed to nor informed about the possibility to change the patrolling intensity (FIX). The experiments were performed at the University of Giessen, Germany between October 2000 and November 2001 and involved 22 female and 78 male students majoring in economics, who participated on a voluntary basis. In most cases, the players within one team knew each other by name. Yet the intensity of personal contact between them, measured by the average number of co-players that have visited each player, differed substantially across teams. With respect to the three different set-ups, no significant differences in the years of study or intensity of private contact were observed.

4. Predictive theories

This section will develop predictions concerning the behavior of the groups of students in the experiment described above. Section 4.1 applies game-theoretic reasoning to predict the individual as well as group behavior in the three different set-ups. In section 4.2, these predictions then analysed and modified or complemented by taking into account evidence from experimental studies.

4.1 Game theoretic predictions

4.1.1 Quota-setting behavior

The efficient fishing strategy for the group as a whole is to extract 5 000 fish in the first six rounds and 6 000 fish in the seventh round. This leaves 8 000 fish in the lake at the beginning of round 8 and therefore ensures the maximum pension of $ 2 000 for each player. The maximum total group earning is thus $ 46 000, which equals $ 9 200 per player. Every deviation from the described strategy will reduce the total group return. If undiscovered defection leads to over-fishing, the CPR can only recover if the quota is reduced in the next round. The optimal quota Q*, that is the quota that maximizes the total possible yield, is given by the following expression:

$Q^* = \max \{0; F_t - R_t\}$

where F_t = number of fish in the lake in the beginning of round t

$R_t = 5\,000$ for $t = 1 \ldots 6$

$R_t = 4\,000$ for $t = 7$

Assuming rational players, the teams can be expected to follow this method of quota-setting.

Prediction 1: All teams will follow the method described above when setting their quota.

4.1.2 Rule-setting behavior, defection and group payoff

After the quota has been set, each player has to decide whether to comply to it or to try to extract more fish. The probability for a defecting player to get caught does not depend on the amount of fish he wants to catch in addition to the allowed quota. Hence:

Prediction 2: Defecting players will try to extract the maximum possible number of 2 000 fish.

Table 2: Simulated payoffs for different patrolling rules and levels of defection

Patrolling rule / player 1's strategy	Individual payoff of player 1 for … defecting co-players (payoff calculated using a discount rate of 0.1 per round)				
	0	1	2	3	4
1 cooperate	9 200 (6 386)	5 731 (4 527)	2 240 (2 094)	1 571 (1 514)	1 222 (1 200)
1 defect	15 268 (11 108)	5 400 (4 934)	3 333 (3 200)	2 500 (2 450)	2 000 (2 000)
2 cooperate	8 850 (6 125)	6 698 (4 780)	3 352 (2 820)	2 140 (1 952)	1 789 (1 673)
2 defect	11 682 (8 383)	7 071 (5 684)	4 029 (3 596)	3 104 (2 869)	2 514 (2 384)
3 cooperate	8 150 (5 604)	6 245 (4 326)	5 475 (3 964)	3 813 (2 928)	3 574 (2 833)
3 defect	7 662 (5 341)	6 856 (4 894)	5 175 (3 909)	4 302 (3 358)	3 522 (2 861)
4 cooperate	7 450 (5 082)	5 917 (4 037)	6 376 (4 396)	5 487 (3 820)	5 779 (4 065)
4 defect	4 750 (3 147)	4 691 (3 115)	4 384 (2 941)	4 169 (2 816)	3 966 (2 714)
5 cooperate	6 050 (4 039)	5 196 (3 451)	5 867 (3 917)	5 653 (3 768)	5 936 (3 963)
5 defect	858 (227)	922 (268)	892 (242)	936 (277)	976 (299)
6 cooperate	4 300 (2 734)	3 985 (2 517)	4 279 (2 720)	4 255 (2 704)	4 297 (2 732)
6 defect	-2 230 (-2 088)	-2 190 (-2 062)	-2 205 (-2 069)	-2 208 (-2 074)	-2 197 (-2 065)

Under the initial rule, the probability of getting caught when defecting is 0.25. Thus any player can increase his expected short-term payoff by defecting. Due to the stochastic nature of the game, the expected long-term payoff from defection cannot be calculated exactly. Table 2 contains estimates for the payoffs from cooperation and defection that an individual player can expect for different patrolling intensities and co-players' behavior. These estimates represent the average payoffs of 10 000 simulated experiments per constellation. Teams are assumed to satisfy prediction 1 in their quota-setting behavior. Two different estimates are presented. While the first figure represents the sum of all payoffs throughout the experiment, the payoffs in parenthesis are discounted using the rate of 0.1 per round. Discounting accounts for the fact that, given the danger of resource extinction, the payoffs become increasingly uncertain the later in the experiment they are expected to occur. The discount factor thus represents the rate of time preference due to uncertainty. The simulated payoffs in Table 2 show that for rule 2, defection represents the strictly dominant strategy. Hence rational players can be expected to defect under the initially installed patrolling intensity. At the same time, the simulation results clearly show that collective defection reduces the expected payoff by 71.6 % from $ 8 850 to $ 2 513 per capita. In order to avoid these losses, the players must try to convince or force each other to cooperate.

MAJ and UNA teams can destroy the incentives to defect by increasing the patrolling intensity. Rule of thumb calculations indicate that, under rule 3 (P = 0.5), a risk-neutral player cannot expect any short-term gains from defection, as the expected payoff is equal to the certain payoff in the case of cooperation. Among risk-averse players, rule 3 is sufficient to induce cooperative behavior. Yet the structure of payoffs in table 2 indicates that any cooperative equilibrium under rule 3 remains fragile. Especially when assuming a positive rate of time preference, any rational player is better off if he defects as soon as he expects one or more of his co-players to defect. The patrolling intensity of rules 4 to 6 is sufficient to destroy all short- as well as long-term incentives to defect. Among these three rules, rule 4 induces a stable cooperative equilibrium at the lowest patrolling costs. By setting rule 4, MAJ and UNA teams can thus expect a certain payoff of $ 7 450 per player. This sum represents a benchmark for these teams.

> *Prediction 3: At minimum, MAJ and UNA teams will achieve a net return of $ 7 450 per person.*

However, if rule 4 is introduced in the first round, each player has to incur patrolling costs of $ 1 750, which is equivalent to 19.0 % of the gross income he expects to earn during the experiment. Therefore MAJ and UNA teams may try to save these costs at least in parts by using other mechanisms than increasing the patrolling intensity to induce cooperative behavior. Threats may be a suitable instrument for this purpose. MAJ and UNA teams can apply the same types of threats as FIX teams. First, they can threaten to set an inefficiently high quota for the rounds following defection. Any higher quota will surely further reduce the expected income of both defecting and cooperative players. Therefore the corresponding threat lacks credibility. Second, the other players can threaten to react by changing from cooperative behavior to defection. In this case, they follow a strategy of loss minimization or “catch as catch can”. The CPR will be depleted very quickly, destroying the prospect of future fishing income as well as the pension. As long as the cooperative players see a chance for the CPR to recover, a change from cooperation to defection is irrational. The corresponding threat is thus not credible either. Consequently, FIX teams do not have any effective instruments to destroy the incentives to defect. Hence:

Prediction 4: FIX teams will witness a significantly higher frequency of defection than MAJ and UNA teams.

Prediction 5: FIX teams will not be able to preserve the CPR from extinction.

Other than FIX teams, MAJ and UNA teams can apply an additional third type of threat to induce cooperative behavior. They may threaten to vote for a higher patrolling intensity if one or more player defect. As the costs of this reaction are moderate and the benefits are substantial, players can credibly threaten to install a high patrolling rule once defectious behavior occurs.[3] This threat is, however, not sufficient to destroy the incentives to defect. Assume that the group agreed to install rule 1 for a start. In this case, a defecting player can earn $ 2 000 instead of $ 1 000 by defecting. If, in the next round, a higher patrolling rule is set, his future income is reduced by the additional patrolling costs. These are higher, the more rounds are left. In addition, the increased patrolling intensity will make it less profitable for him to defect in the next rounds. If he complies to the quota, he can earn only $ 600 instead of $ 1 000 in the round following his defection. After that, a gross fishing income of $ 1 000 is feasible again. Hence his net gain from defection in round t is given by the following formula:

$$\text{Net gain} = 600 - \Delta C_R \times (7 - (t+1))$$

where ΔC_R represents the change in patrolling costs per round.

For any player who expects one or more of his co-players to defect, $\Delta C_R = 0$, because the patrolling intensity will be increased regardless of his own action. Thus he faces massive incentives to defect. Now consider the case where the individual

[3] Even the player who defected in the previous round has the same interest in a higher rule as the rest of the group. By changing the rule he reduces the danger that the CPR is deplete due to defection of other players. The fact that he makes his own defection in the future less profitable only comes to a minor disadvantage and is outweighed by the increase in expected income from avoiding extinction. In sum, the obstacles for a change in patrolling intensity are moderate.

player expects all his co-players to comply to the quota. Assuming rule 3 is set as a reaction to defection, defecting pays if there are less than 4 rounds left to play. If, however, players realize that under rule 1, defection is rational in rounds 4-6, they must introduce a higher patrolling rule in the beginning of round 4 to effectively fight defection. As a result, the net gain from defecting in round 3 equals \$ 600, as $\Delta C_R = 0$. Hence control has to be introduced in round 3. If this is again anticipated, a player can try to increase his income by defecting in round 2. In the end, this line of reasoning leads to the following prediction:

> *Prediction 6: MAJ and UNA teams will introduce patrolling rule 3 or higher in round 1.*

4.2 Evidence from earlier experiments

When predicting human behavior, game-theorists often overestimate the cognitive capacities of the players and/or their willingness to use them (e.g., *Anderson, Goerree and Holt*, 1998). In the current experiment, the optimal fishing strategy –at least for round 1 to 6 – is easy to identify. As the players have to discuss it openly before settling the quota, they can be expected to recognize and set the optimal quota as predicted in prediction 1. Thus the evidence from preceding experiments does not cast any doubt on the tenability this prediction. At the same time, it is much more complicated to estimate the expected payoffs under different rules (see Table 2). Rule-of-thumb calculations can, however, help the player to find out that, on average, defection does not increase the payoff if the probability of getting caught is 50 % or higher (i.e. under rule 3 to 6) but brings substantial additional payoffs, at least in the short-run, under rule 1. For the initial rule 2, rule of thumb calculations will return a moderate incentive to defect. In sum, the rough incentive structure of monetary incentives should be clear to the players.

Whether rule 3 is sufficient to induce cooperative behavior in MAJ and UNA teams depends on the players' attitude towards risk as well as their attraction to chance (e.g., *Albers et al.*, 2000). In different series of lottery experiments, *Holt and Laury* (2000, 2000a) found that the experimentees were on average risk-averse regardless of whether they faced low or high, real or hypothetical payoffs. On the other hand, participants of experiments tend to systematically overestimate their own chance of winning in lotteries (e.g., *Camerer* 1995; *Thaler* 1991). In addition, *Albers et al.* (2000) showed in a series of experiments that the attraction to chance is an independent and positive source of utility for people having to choose between a certain payoff and a lottery. In sum, the results of lottery experiments give no clear answer to the question whether rule 3 will be sufficient to induce cooperation in MAJ and UNA teams.

Game-theoretic reasoning systematically underestimates the level of voluntary cooperation in social dilemma games. This is especially true in those cases, where players are allowed to communicate (e.g., *Ostrom, Walker, Gardner*, 1992; *Weimann*, 1994). This behavior can partly result from a general predisposition to cooperate and a preference for fair results (e.g., *Falk, Fehr and Fischbacher*, 2000; *Fehr and Gächter*, 2000). In the current experiment, the quota is not only discussed but formally voted on. This makes the quota more binding (e.g., *Vanberg*, 1988) and can thus further increase the degree of cooperation. Therefore defection can be expected to be less frequent than predicted in section 4.1 even if the patrolling intensity is low. This in turn casts doubt on prediction 4 and prediction 6. At the same time, previous single choice experiments have shown a termination effect, that is a substantial though not full deterioration of cooperative behavior in the last round (e.g., *Weimann*, 1994; *Ledyard*, 1995; *Ehrhart*, 1997, S. 49 - 52). This leads to prediction 7:

> *Prediction 7: FIX teams will witness a significant increase in defection in the last round.*

Consequently, the CPR is in danger of extinction, giving support to prediction 5.[4]

A common result of all experiments on social dilemma situations is that players very seldom choose extreme actions, even if these are rational. Especially in public good experiments, very few players contribute all private resources or none of them to the provision of the public good (e.g., *Anderson, Goerree and Holt*, 1998). Warm-glow effects are found to be one determinant of the players' attraction to "moderate defection" (e.g., *Andreoni*, 1995; *Palfrey and Prisbrey*, 1997). Assuming that the players in the current experiment are guided by the same motives, defecting players may abstain from extracting the maximum possible 2 000 fish. This casts doubt on the tenability of prediction 2.

Finally, all experimental studies report a very wide dispersion of group behavior. In single choice experiments, some groups are found to cooperate in the vast majority of cases, while others show extensive defection (e.g., *Isaac, Walker and Thomas*, 1984, 1994; *Gardner, Moore and Walker*, 1997). As these teams are placed in an identical environment with identical incentives, the different degrees of cooperation observed must be caused by differences in group characteristics, such as the players' attitude towards risk or their preference for fairness. While

[4] Experiments have furthermore shown that the degree of cooperation is higher if players are given the opportunity to punish their defecting co-players. The players are willing to punish defecting players even if this caused substantial private costs (e.g., *Chen and Plott*, 1987; *Frey and Bohnet*, 1995; *Fehr and Gächter*, 2000). In the current experiment, the players do not have the possibility to formally punish single players. They cannot effectively apply informal sanctions either because they do not know who defected as long as the defecting players do not get caught. In sum, sanctioning cannot help the teams to induce cooperation.

some teams are able to reach a cooperative solution under the given set of rules, the same rules prove inappropriate for other teams. In single-choice experiments, the latter type of team will perform poorly because it does not have a suitable instrument at hand to resolve the social dilemma. Teams in double-choice experiments, on the other hand, are given a suitable instrument as they are allowed to change the rules, if this proves necessary. As a result, the average economic performance in double-choice experiments can be expected to be higher than in otherwise identical single-choice experiments. The evidence from the experiments by *Sato* (1987) and *Carpenter* (2000) support this conclusion. Furthermore, the easier it is for a team to change the rules, the more successful it can be expected to be. In the context of the current experiment, these considerations lead to a final prediction[5]:

> *Prediction 8: MAJ teams will reach the highest average payoff, followed by UNA teams. The average payoff under both set-ups will exceed the one reached by FIX teams.*

5. Results

5.1 Quota-setting behavior

All teams passed quotas in all rounds. In 89.9% (80.0% in the MAJ, 95.2% in the UNA and 92.9% in the FIX set-up) the quota was passed in an unanimous vote. In round 1 to 6, 88.2 % of all quotas were set in accordance with prediction 1. Regardless of the set-up, all teams set the efficient quota of 1 000 fish per capita in round 1, thereby clearly indicating that they identified the group-efficient fishing strategy. As long as the number of fish did not drop below 10 000 in round 1 to 6, 81 out of 82 quotas were set efficiently. In those situations where the lake contained less than 10 000 fish, 65 % of the quotas were set according to prediction 1. All deviations from the efficient quota led to over-fishing. In round 7, 14 out of 19 teams realized that a higher quota has to be set. Four teams continued to har-

[5] This prediction is also backed by theoretical considerations following the evolutionary approach. These show that the economic success of a group of individuals exploiting a CPR crucially depends on the composition of the group. Only if the share of individuals who have a strong preference for the cooperative solution is sufficiently large, will these be rewarded by above-average payoffs and rationally stick to their cooperative strategy. If, however, the population contains fewer so-called cooperators, they will reach below-average payoffs and thus vanish or change to defection (e.g., *Sethi and Samanathan*, 1996). Institutional change is the only possibility to escape the tragedy of the commons in this case. As only MAJ and UNA teams are given this opportunity, only they can reach a cooperative solution regardless of the share of cooperators within them, while FIX teams can only expect a cooperative solution in those cases when the share of cooperators is sufficiently large.

vest 5 000 fish from a lake of 10 000, while only one teams slightly over-fished an already decimated population. In total, 86.2 % of all quotas were set efficiently. In sum, the experimental evidence gives strong support for prediction 1.[6]

5.2 Rule-setting behavior and defection

After setting a quota, MAJ and UNA teams have to decide about the patrolling intensity. In the beginning of the game, six of these teams abolished patrolling completely, four left the rule unchanged and only two teams introduced rule 3 (see Table 3).

Table 3: Patrolling rule and intensity in MAJ and UNA groups

Group	Round							Average patrolling intensity
	1	2	3	4	5	6	7	
MAJ_1	1	1	1	1	1	1	1	0.0
MAJ_2	3	3	3	3	3	3	3	50.0
MAJ_3	2	3	3	2	2			35.0
MAJ_4	2	3	3	1	1	3	3	32.1
MAJ_5	1	2	3	3	2	3	2	32.1
MAJ_6	1	1	1	1	1	1	1	0.0
Av. patrolling intensity	*16.7*	*29.2*	*33.3*	*20.8*	*16.7*	*30.0*	*25.0*	*24.5*
UNA_1	1	1	1	1	1	1	1	0.0
UNA_2	1	1	1	1	2	2	2	10.7
UNA_3	1	1	1	1	1	1	1	0.0
UNA_4	3	3	3	3	3	5	4	57.4
UNA_5	2	4	4	3	3	2	3	51.2
UNA_6	2	2	1	3	3	3	3	35.7
Av. patrolling intensity	*20.8*	*23.6*	*19.5*	*25.0*	*29.2*	*30.8*	*32.0*	*25.8*

Only three ballots for a stricter rule were called but none were passed. Prediction 6 does thus not prove tenable. All 12 teams which had the right to change the patrolling rule made use of this right at least once during the experiment. Three MAJ and two UNA teams changed the patrolling rules in the first round and stuck to the newly introduced rule throughout the entire experiment. Four of these teams chose rule 1, while one MAJ team chose rule 3. The other teams changed the rule more frequently, at most 5 times. The strictest patrolling rule implemented

[6] The efficiency of rule setting is not found to differ between set-ups. The fact that the overall frequency of efficient quotas was slightly higher in FIX teams results from the fact that these teams faced a full lake more frequently than MAJ and UNA teams.

was rule 5. The average patrolling intensity throughout the entire game – measured by the probability of discovering defection – was 24.5 % for MAJ and 25.8 % for UNA teams and thereby came very close to the patrolling intensity of rule 2 under which FIX teams were forced to play.

MAJ teams called on 23 ballots with 14 of them being passed. Under the UNA set-up, 41 ballots on changing the rules were called and 12 of them were passed. As predicted in section 4.2, players in the UNA set-up faced higher obstacles when trying to introduce their preferred patrolling rule. Yet the difference in restriction compared to the MAJ set-up is moderate, since all UNA teams were able to change the patrolling intensity and in two cases even introduced stricter rules than the MAJ teams did. In OLS regressions, neither the average patrolling intensity nor the intensity set in round 1 is found to depend on the ascertained group characteristics, i.e. the number of semesters, the share of female players or the intensity of pre-game contact between team members. The corresponding coefficients of correlations never exceed 0.32.

MAJ teams increased the patrolling intensity six times and lowered it four times, UNA teams five respectively four times. In both set-ups the lowest average rule per round was observed in round 1. When looking at those seven teams who changed the rule beyond round 1, only two of them reduced the patrolling intensity beyond the level of round 1 during the experiment. At the same time, all of them played under stricter patrolling at least once during the experiment. Tracing the rule-changing behavior beyond the first round reveals no systematic pattern for MAJ teams but a significant increase in patrolling intensity for UNA teams. The Spearman’s coefficients of correlation between the round number and the average patrolling intensity takes on the values of 0.07 respectively 0.89.

The average number of defections per round was 0.79 for MAJ, 0.76 for UNA compared to only 0.64 for FIX teams (see Table 4). This result stands in sharp contradiction to prediction 4. The teams in the MAJ and UNA set-up were not able to use the right to change the patrolling intensity to reduce defection. When tracing defection across rounds, no termination effect was observed in FIX teams. Thus prediction 7 does not prove tenable either. In 46.5 % of all attempts to defect (57.6 % in the MAJ, 50.0 % in the UNA and 33.3 % in the FIX set-up), players planned to extract less than the maximum possible amount of 2 000 fish. In some cases, the fishing plans exceeded the quota by no more than 10 %.[7] This behavior clearly violates the aim of payoff maximization underlying prediction 2. In addition, teams who played under a low patrolling rule did not, on average, witness more defection than teams playing under a high patrolling intensity. In particular,

[7] This behavior was especially frequent in the FIX_3 team and explains why this team was able to save the CPR from extinction despite the low patrolling intensity and 17 attempts to defect.

those four teams who played all rounds without any patrolling did not witness more defectious behavior than the other MAJ and UNA teams.

Table 4: Group characteristics and group performance

group	average patrol-ling intensity	group pension	net group payoff	number of de-fections	changes in rule	Average number of semester	average number of visits	number of female players
MAJ_1	0.0	8 000	44 800	1	1	7.00	3.40	0
MAJ_2	50.0	10 000	44 950	0	1	5.40	2.80	2
MAJ_3	35.0	0	23 350	9	2	7.20	2.40	2
MAJ_4	32.1	2 400	29 510	14	4	1.00	2.60	1
MAJ_5	32.1	5 250	29 750	9	5	4.20	2.00	1
MAJ_6	0.0	10 000	46 000	0	1	11.20	0.80	2
Average	*24.5*	*5 941.67*	*36 393.33*	*5.50*	*2.33*	*6.00*	*2.33*	*1.33*
UNA_1	0.0	9 950	41 420	7	1	9.00	3.40	0
UNA_2	10.7	10 000	43 060	4	1	9.80	0.00	2
UNA_3	0.0	10 000	46 000	1	1	5.00	3.00	0
UNA_4	57.4	10 000	36 490	12	3	7.20	2.40	0
UNA_5	51.2	10 000	40 150	4	4	6.00	0.60	1
UNA_6	35.7	10 000	39 370	4	2	6.20	1.20	2
Average	*25.8*	*9 991.67*	*41 081.67*	*5.33*	*2*	*7.20*	*1.77*	*0.83*
FIX_1	25.00	10 000	36 050	9		9.40	2.00	0
FIX_2	25.00	10 000	45 649	0		2	2.80	1
FIX_3	25.00	6 000	40 628	17		7.40	1.00	2
FIX_4	25.00	10 000	44 647	1		7.60	2.00	0
FIX_5	25.00	10 000	43 846	2		10.40	0.20	3
FIX_6	25.00	10 000	43 545	2		7.00	3.20	0
FIX_7	25.00	7 900	41 304	5		7.60	0.00	1
FIX_8	25.00	10 000	44 643	0		1.00	2.40	2
Average	*25.00*	*9 237.50*	*42 539*	*4.50*		*6.55*	*1.70*	*1.13*

The rule-changing behavior beyond the first round was significantly influenced by the intensity of defection in the preceding round. All 11 increases in patrolling intensity beyond round 1 followed a round in which at least one player defected, while rounds with 100 % cooperation were never followed by an increase in rule. Those eight cases in which the patrolling intensity was reduces are less straightforward to interpret. Only three of these changes were preceded by full cooperation in the previous round. In five cases, the patrolling intensity was reduced despite prior defection. The players agreed to set a very low quota even though this step increased the incentives to defect massively especially because they simultaneously set a low quota to give the CPR a chance to recover. The intra-group conversation revealed the following justification for this step: Some players argued in favor of the reduced patrolling intensity because otherwise the patrolling costs would exceed the fishing income in that round. The teams followed this course of argumentation and intensified moral suasion to induce cooperative behavior.

5.3 Economic performance

As illustrated in table 5, only very few teams were able to keep the pool at its original size of 10 000 fish throughout the first six rounds as would have been efficient. At the same time, the degree of reduction in stock differed for the three setups.

Table 5: Common pool size at the beginning of round 2 to 7

group	Pool size at the beginning of round					
	2	3	4	5	6	7
MAJ_1	10 000	10 000	10 000	10 000	10 000	10 000
MAJ_2	10 000	10 000	10 000	10 000	10 000	10 000
MAJ_3	10 000	10 000	2 000	5 600		
MAJ_4	9 200	7 900	9 200	2 400	4 780	5 960
MAJ_5	6 600	6 400	6 800	9 600	2 000	3 000
MAJ_6	10 000	10 000	10 000	10 000	10 000	10 000
Average	*9 300*	*9 050*	*8 000*	*7 933.33*	*7 356*	*7 792*
UNA_1	10 000	9 000	9 600	9 300	7 600	5 400
UNA_2	10 000	8 000	9 200	9 280	10 000	8 000
UNA_3	10 000	10 000	10 000	10 000	10 000	10 000
UNA_4	8 000	9 600	10 000	9 000	4 600	9 200
UNA_5	6 000	8 000	10 000	10 000	10 000	8 000
UNA_6	8 000	6 400	9 760	10 000	10 000	10 000
Average	*8 666,67*	*8 500*	*9 760*	*9 596,67*	*8 700*	*8 433,33*
FIX_1	8 000	10 000	10 000	10 000	6 000	8 000
FIX_2	10 000	10 000	10 000	10 000	10 000	10 000
FIX_3	9 600	10 000	9 200	10 000	10 000	10 000
FIX_4	10 000	10 000	10 000	10000	10 000	10 000
FIX_5	10 000	8 000	10 000	10 000	8 000	8 400
FIX_6	10 000	10 000	9 800	10 000	10 000	10 000
FIX_7	10 000	10 000	10 000	8 000	8 400	9 800
FIX_8	10 000	10 000	10 000	10 000	10 000	10 000
Average	*9 600*	*9 666,67*	*9 857,14*	*9 714,29*	*8 914,29*	*9 457,14*

Three out of the six MAJ teams reduced the stock below 4 000 fish, while neither UNA nor FIX teams trespassed this mark. At any round, the average stock of fish of FIX teams exceeded the average stock of MAJ and UNA teams. Except for one MAJ team, all teams managed to keep the pool of fish from extinction. Only two MAJ teams left sufficient fish in the lake to be granted the maximum possible pension, while the other MAJ teams had to incur losses in pensions up to 75 % (respectively 100 %). On the other hand, all UNA teams and six out of eight FIX teams reached the full pension. The pensions of MAJ teams are significantly smaller than those of UNA 25 teams (α= 0.05) as well as the FIX teams (α= 0.1). At the same time, no difference was found between UNA and FIX teams. The above-average performance of FIX teams stands in sharp contradiction to prediction 5.

MAJ teams achieved net payoffs ranging from \$ 23 350 to the maximum possible yield of \$ 46 000, with an average of \$ 36 393. UNA teams performed better on average (\$ 41 083), with payoffs ranging between \$ 36 490 and \$ 46 000. Three MAJ and one UNA teams ended up with a group payoff that was below the one they could have expected when setting rule 4 (\$ 37 250). This result stands in sharp contradiction to prediction 3. The FIX teams reached payoffs between \$ 36 050 and \$ 45 659 and performed best on average (\$ 42 539). Their net payoff is significantly larger than for MAJ teams(α = 0.1). This result strongly contradicts prediction 8. OLS-regressions show that the differences in economic performance do not result from differences in the ascertained group characteristics. All corresponding coefficients of correlation were smaller than 0.3. Those four teams which introduced rule 1 in the first round and stuck to it reached an average payoff of \$ 44 555. The group payoff of the team which played the entire experiment under rule 3 amounted to \$ 44 950. These five teams achieved higher payoff than the average FIX teams as predicted in section 4. Teams with more frequent changes in patrolling intensity only earned an average of \$ 34 590 (\$ 27 537 for MAJ teams and \$ 39 880 for UNA teams), which is substantially less than the average payoff of FIX teams. An OLS regression for MAJ and UNA teams between the number of changes in rules and the net payoff produced a significantly negative slope (α = 0.05; R^2 = 0.424).

6. Discussion

All groups, regardless of whether they were allowed to change the institutional settings or not, witnessed a degree of cooperation throughout the entire game that substantially exceeded the one predicted by game theory. In this respect, the results of the current experimental study stands in line with previous experiments on social dilemma situations. In those cases where defection occurred, the players in almost 50 % of the cases extracted less than the maximum possible 2 000 fish. This result suggests that the corresponding players wanted to defect without heavily diminishing the CPR in the case of success. This behavior supports the notion put forth by *Albers et al.* (2000), who suggest that the mere existence of chance constitutes a source of positive utility which is independent of the structure of payoffs.

As a consequence of the high degree of cooperation, the groups, especially FIX teams, reached higher payoffs than predicted. Regarding the low average patrolling intensity, two explanations seem possible. First, the players were guided by a predisposition to cooperate. This would also explain the missing termination effect in FIX teams. Second, the possibility to communicate allowed them to successfully use moral suasion and threats to reduce defection. Therefore, compared to the players in previous double-choice experiments who were not allowed to communicate, teams in the current experiment had to rely less heavily on externally enforced institutional restrictions. This may explain why FIX teams in the current study were much more successful than the teams in previous experimental studies as long as they were not allowed to install an efficient set of rules.

Like in previous double-choice experiments, the groups made frequent use of the right to change the institutional settings. In the current experiment, all teams which were allowed to change the institutional settings made use of this possibility at least once during the experiment. This gives further support to the notion that human individuals regard institutional change as a method of dissolving social dilemma situations. At the same time it shows that the majority rule and even unanimity only pose moderate obstacles for institutional change. This result supports *Brennan and Buchanan* (1985) who argue that groups of human individuals manage to introduce institutional change even under unanimity rule if the proposed rules are considered fair.

In one central aspect, the results of the current experiment do, however, stand in sharp contrast to those of previous double-choice experiments. Other than the teams in the experiments performed by *Sato* (1987) and *Carpenter* (2000), the MAJ and UNA teams in the current experiment were not able to capitalize the right to change the institutional settings. Instead of having a positive impact on the average payoff, the additional option that these teams had compared to FIX teams reduced the average payoff. This result heavily contradicts the wisdom of textbooks on the economic theory of decision making. These state that an additional option never reduces the payoff of an economic agent, at worst it leaves the payoff unchanged. Neither a lack in self-control (e.g., *Thaler*, 1991, S. 77 - 90) nor problems of self-commitment (e.g., *Fudenberg and Tirole*, 1991, S. 74 - 77) can explain why the additional option led to losses in average payoffs.[8] As, on aver-

[8] In addition, none of the players in MAJ and UNA teams expressed any discontent about the fact that they were given the right to change the rules.

age, MAJ and UNA teams played under the same patrolling intensity as FIX teams, excessive patrolling costs cannot serve as an explanation either.

The author can offer the following explanation for this puzzling result. In the experiments of *Sato* (1987) and *Carpenter* (2000), all groups had played a number of rounds under a fixed set of rules. During these rounds, they had the opportunity to learn about the social dilemma situation and the behavior of their co-players. And only after they had learned about the functioning of the game under a given set of rules, they were allowed to change the rules and learn about the functioning of different rules. In the current experiment, MAJ and UNA teams had the right to change the rules from the very beginning of the game. The teams had to find the "correct" rule in a trial and error process without any experience from earlier rounds. Some teams managed to pick the patrolling rule which was suitable to induce cooperation in their particular case in the first round. Other teams chose an insufficiently low rule. The negatively significant relationship between the frequency of changes in rule and net payoff clearly indicates the following: The longer the process of trial and error, the lower the payoffs. The frequency of changes in patrolling intensity proved to be a negative function of the number of semesters the students have studied ($\alpha = 0.05$). This indicates that experienced teams have accumulated knowledge in economic reasoning which makes it possible for them to better comprehend the functioning of the experiment a priori than groups of inexperienced students.

Regarding the lack of experience, all teams took a high risk at the beginning of the game when they introduced very low patrolling rules. The question that needs to be answered is why they took this high risk even though their knowledge on the mechanism of the game and their co-players' behavior was very limited? Possibly, the players did not consider the low patrolling intensity a risk because they overestimated the predisposition of their co-players to cooperate. Such "wishful thinking" is reported especially for players who are themselves predisposed to cooperate (*Offerman, Sonnemans and Schram*, 1996). This "wishful thinking" may have been strengthened by the fact that the quota was passed unanimously in the overwhelming number of cases and was therefore regarded as an agreement of compliance. Another explanation is that calling for a ballot as well as voting for intensive patrolling can be interpreted as a declaration of ex ante mistrust. This can destroy an initial climate of trust and predisposition to coordination and thereby increase the co-players' readiness to defect. In addition, the implicit declaration of mistrust may be unpopular among players who know each other. Following this line of argumentation, even players who were uncertain whether the low patrolling intensity will be sufficient faced an incentive not to voice their doubts. A strong antipathy to pay for patrolling may serve as a further explanation for the low patrolling intensity MAJ and UNA teams set at the beginning of the experiment. This reluctance was openly expressed by teams who were confronted with a substantial degree of defection but failed to intensify the patrolling intensity. As the previous experiments by *Sato* (1987) and *Carpenter* (2000) did not account for costs of enforcing institutions, the players in their experiments were more readily willing to impose heavy restrictions on the access to the CPR.

7. Conclusion

Since *Hardin*'s tragedy of the commons, social dilemma situations have been subject of numerous theoretical and empirical articles. Therein institutional change was identified to be a very important measure to resolve the social dilemma situation and reach efficient results. So far, only very few experimental studies have been performed to investigate the ability of groups of human individuals to apply this measure effectively. This paper presents a newly designed double-choice experiment that can give further insight into the human behavior in situations of institutional choice. It differs from preceding double-choice experiments in three fundamental features. First, the players are not given the opportunity to learn about the basic mechanisms of the game before being allowed to change the institutional settings. Second, they are allowed to communicate. Third, the experimental set-up accounts for the costs of enforcing institutional restrictions on the access to the CPR.

The major result of the current experimental study can be summarized as follows. Like in most experiments on social dilemma situations, the degree of voluntary cooperation was substantially higher than predicted by game theory. Under moderate patrolling, the FIX teams, who were not allowed to change the institutions, reached near-efficient results. Thereby the performed far better than the control-groups of previous double-choice experiments. This result underlines the prominent role of communication in resolving social dilemma situations. The experiment furthermore clearly shows that those groups who were allowed to change the rules of their interaction made use of this opportunity to try to resolve the social dilemma situation. This indicates that humans regard institutional change as an appropriate means of influencing the economic outcome according to their preferences. On the other hand, MAJ and UNA teams reached payoffs which were on average lower than those of the control groups, who were not given the right to change the rules. Following elementary economic reasoning, the opposite result could have been expected. Though the author offers a first plausible explanation for this puzzling result, this result casts doubt on the ability of human individuals to correctly predict the impact of institutional change on human behavior and economic outcomes. This in turn suggests that the capability of groups of human individuals to apply institutional change to resolve social dilemma situations is limited. Despite giving new insight into this interesting field of research, the current experiment clearly shows that far more double-choice experiments are necessary to substantially improve our knowledge of this topic.

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